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Professor A. S. EDDINGTON, F.R.S., PRESIDENT, in the Chair.

The Rev. Ivo Francis Henry Carr Carr-Gregg, M.A., Astley Vicarage, Nuneaton;

William George Gould, Lieut. R.N.R., A.M.I.N.A., L.C.C. Paddington Technical Institute;

Stanley Robert Hewitson, B.Sc., Head Master, Royal Hospital School, Queen's House, Greenwich, S.E. 10;

Jan Krassowski, Ph.D., Director of the Observatory, Warsaw, Poland;

Capt. Charles John Moorsom (late R.G.A.), Civil Engineering Section of the Imperial College of Science and Technology, South Kensington, S.W. 7;

Capt. Austin Henry Quince, Lieut. R.N.R., H.M.S. *Worcester*, Greenhithe, Kent; and

Robert Stoneley, M.A. (Camb.), Mathematical Department, University of Sheffield,

were balloted for and duly elected Fellows of the Society.

The following candidate was proposed for election as a Fellow of the Society, the name of the proposer from personal knowledge being appended:—

Edward Osborne Fountain, B.A., M.D., Crossley House, Ruislip, Uxbridge (proposed by L. J. Comrie).

Antonio Abetti, Professor of Astronomy and Director of the Arcetri Observatory, Florence;

Heber D. Curtis, Director of the Allegheny Observatory, University of Pittsburgh, Pennsylvania, U.S.A.;

Ejnar Hertzsprung, Chief Assistant, Observatory, Leiden, Holland; and
Georges Lecointe, Director of the Royal Observatory of Belgium, Uccle, Belgium,

were proposed by the Council as Associates of the Society.

Fifty-seven presents were announced as having been received since the last Meeting, including, amongst others:—

Five photographs of nebulæ, etc., taken with the 30-inch reflector of the Cordoba Observatory (enlarged transparencies), presented by Professor C. D. Perrine.

Note on Barnard's Observations of Nova Ophiuchi No. 2 (Hind, 1848) and of Nova Persei No. 2 (Anderson, 1901). By H. H. Turner, D.Sc., F.R.S., Savilian Professor.

1. In his interesting paper (*M.N.*, 81, 167) on Hind's Nova of 1848, Barnard suggests a period of 30 or 40 days for the fluctuations. A period of 27·3 days seems to fit the observations well. Taking the phases $27^{d}\cdot3\,\theta^{\circ}/360^{\circ}$ from dates

$$2422096\cdot0 + 27\cdot3\,E$$

as in the second column of the following table, and calculating the fourth column from the formula

$$C_1 = 0\cdot18\cos(\theta - 180^{\circ}) - 0\cdot04$$

(the second and third harmonics being small), we get a fair accordance, as shown by the column $O - C_1$. The first column gives the J.D. of observations in Central Standard time; for G.M.T. add 6^{h} or $0^{d}\cdot25$. The column "Obs." is the difference of Barnard's observation from his mean value 12·35, expressed in units of 0·01 mag., and the same unit is of course used for C.

2. When the $O - C_1$ residuals were restored to time-sequence, a period of 54·6 days was suggested. The coefficient is 0·10, and in the column C_2 a small constant 0·02 is also included. The column $O - C_1 - C_2$ shows some reduction in the mean of the errors: and we can make a further reduction if we correct all the observations of 1919 by +0·03 and those of 1920 by −0·04 mag., there being a systematic difference between the two sets. But these two subsidiary hypotheses are more doubtful than the main assumption of 27·3 days.

TABLE I.

Suggested Period of 27^{d}·3 in Nova Ophiuchi No. 2.

J.D.	Phase. d	Obs.	C_1.	O−C_1.	C_2.	O−C_1−C_2.	Mean Values.
2481·5	3·3	−25	−17	− 8	+ 5	−13	−17
2509·4	3·9	−35	−15	−20	− 7	−13	−17
2127·4	4·1	− 5	−15	+10	− 6	+16	+19
2155·4	4·8	−15	−12	− 3	+ 4	− 7	− 4
2101·5	5·5	+ 5	−10	+15	+ 3	+12	+15
2129·4	6·1	− 5	− 7	+ 2	− 5	+ 7	+10
2458·5	7·6	+15	− 1	+16	− 4	+20	+16
2131·4	8·1	− 5	+ 2	− 7	− 2	− 5	− 2
2460·5	9·6	+15	+ 7	+ 8	− 1	+ 9	+ 5
2488·5	10·3	+15	+ 9	+ 6	− 1	+ 7	+ 3
2106·5	10·5	− 5	+10	−15	− 2	−13	−10
2080·5	11·8	+15	+13	+ 2	+ 2	0	+ 3
2162·4	11·8	+ 5	+13	− 8	− 3	− 5	− 2
2547·3	14·5	+35	+14	+21	− 5	+26	+22
2465·5	14·6	+25	+14	+11	+ 4	+ 7	+ 3
2138·4	15·1	+15	+13	+ 2	+ 5	− 3	0
2493·5	15·3	+ 5	+13	− 8	− 7	− 1	− 5
2085·6	16·9	+15	+10	+ 5	+ 7	− 2	+ 1
2141·4	18·1	− 5	+ 6	−11	+ 8	−19	−16
2551·4	18·6	− 5	+ 4	− 9	− 9	0	− 4
2087·5	18·8	− 5	+ 3	− 8	+ 8	−16	−13
2524·4	18·9	+15	+ 3	+12	+13	− 1	− 5
2115·5	19·5	−25	0	−25	−10	−15	−12
2470·5	19·6	+15	− 1	+16	+ 9	+ 7	+ 3
2472·5	21·6	+ 5	− 6	+11	+ 9	+ 2	− 2
2117·5	21·5	−25	− 6	−19	−11	− 8	− 5
2145·4	22·1	− 5	−10	+ 5	+ 8	− 3	0
2176·3	25·7	−25	−22	− 3	−10	+ 7	+10
		±390		±286		±244	±224

3. If this period is confirmed, it seems probable that the fluctuations have lengthened since the outburst in 1848, for the fluctuations which follow the appearance of a nova are usually of a few days' period only. But as between 1919 and 1920 no change in period is discernible from the present evidence.

4. Barnard has, however, made a valuable series of observations of Nova Persei No. 2 (Anderson), given in *M.N.*, **80**, 712–715, which enables us to examine the behaviour of a nova nearer its outburst (16 to 20 years from discovery). A casual inspection suggested periods near 30 days and near 6 days, and it was determined to make a systematic examination, by Fourier analysis, of this series. The terminal dates are 2421207 and 2422405, differing by 1200 days, an interval which contains about 200 periods of 6 days. A direct attack on the problem would thus involve the examination of all divisors up to 200, an amount of work which is to be evaded if possible. The method of evasion corresponds to that of the use of a spectroscope of low resolving power in the first instance, to see whereabouts a bright line, if any, may be: and then the use of a higher-power instrument to give precision to the determination.

5. Resolving power corresponds to length of series, and we must thus break up the series of 1200 days into shorter lengths. A length of 200 days was adopted, since there are roughly three annual series of about this length, if we omit the few observations in 1916–17. To simplify the work as much as possible, each presumed cycle was divided into 4 parts only. In previous work of this kind a cycle has usually been divided into 12 parts, which gives a sensibly correct value for the coefficient. In using so small a number of parts as 4, we naturally diminish the available coefficient. But the diminution is slight, as a simple example will show. Suppose we have a series of 12 terms

$$0,\ +5,\ +\mathrm{F},\ +10,\ +\mathrm{F},\ +5,\quad 0,\ -5,\ -\mathrm{F},\ -10,\ -\mathrm{F},\ -5,$$

where F is written for $10 \sin 60^\circ = 5\sqrt{3}$. By the usual 12-term analysis we should get a coefficient 10. If now we group the 12 terms in 4 sets of 3, beginning with the last, we get the 4 mean values

$$0,\quad 9{\cdot}1,\quad 0,\quad -9{\cdot}1,$$

which give a coefficient 9·1 instead of 10·0. The diminution is not serious enough to interfere with the main (exploring) purpose of the analysis.

6. In Table II. it will suffice to give the values of $\mathrm{R}^2 = a^2 + b^2$ (where a and b are the coefficients of $\sin\theta$ and $\cos\theta$) for the three groups of 200 days. The first cycle of $33^{\mathrm{d}}{\cdot}3$ (divisor 6) had for mean values of the 4 portions

$$13{\cdot}59,\quad 13{\cdot}80,\quad 14{\cdot}12,\quad 13{\cdot}61,$$

and the first harmonic is thus

$$-0{\cdot}265\cos\theta + 0{\cdot}095\sin\theta.$$

The value of R^2 is thus $(0{\cdot}265)^2 + (0{\cdot}095)^2 = {\cdot}079$, which is written as 79.

TABLE II.

Intensities of Periodicities for Nova Persei No. 2, deduced from the Three Sets of Barnard's Observations.

Divisor.	Period.	1917–18.	1918–19.	1919–20.	Sums. (1).	Sums. (2).
	d					
3	66·7	32	11	28	71	50
4	50·0	36	8	82	126	102
5	40·0	22	3	34	59	44
6	33·3	79	30	9	118	65
7	28·6	104	9	0	113	44
8	25·0	26	2	3	31	14
9	22·2	77	12	6	95	44
10	20·0	31	6	23	60	39
11	18·2	16	3	11	30	19
12	16·7	6	24	2	32	28
13	15·4	69	6	2	77	31
14	14·3	36	12	22	70	46
15	13·3	6	12	13	31	27
16	12·5	73	22	7	102	53
17	11·8	8	1	5	14	9
18	11·1	84	30	1	115	59
19	10·5	60	22	7	89	49
20	10·0	48	8	14	70	38
21	9·5	9	2	4	15	9
22	9·0	50	1	12	63	30
23	8·6	50	14	25	89	56
24	8·3	33	26	0	59	37
25	8·0	9	20	63	92	86
26	7·7	8	2	6	16	11
27	7·4	40	7	14	61	34
28	7·1	56	37	49	142	105
29	6·9	33	7	16	56	34
30	6·7	30	12	8	50	30
31	6·5	58	0	6	64	25
32	6·3	108	20	60	188	116
33	6·1	8	28	0	36	31
34	5·9	16	5	17	38	27
35	5·7	24	5	14	43	27
36	5·5	34	11	17	62	39
37	5·4	56	5	6	67	30
Mean	...	42	12	14	68	40

7. A mere glance at Table II. suffices to show that the numbers of the third column are sensibly larger than those of the fourth and fifth: in other words, the results after 1918 March are sensibly different from those before. This cannot be attributed to the observer. It is true that the number of comparisons per night increases slightly along the series, being 3·5 in 1916–17, 6·2 in 1917–18, 7·0 in 1918–19, and 8·1 in 1919–20. This greater assiduity, and perhaps a little greater familiarity with the field of stars, might account for a *slightly* reduced mean deviation: but the change in mean of R^2 is serious, being 3 to 1, steadily over the long series of divisors. This sudden reduction in the fluctuations of the star seems noteworthy as a physical fact.

8. But it has also a special consequence for our exploring work. Were the three columns homogeneous, it would be natural to take their sum, as in the fifth column under head (1), as the periodogram. But the general principle of the periodogram method is to select terms in which R^2 is large compared with its mean value. It is clearly unsound to add together for this purpose three columns with such different mean values. It is preferable to divide the first by 3 before taking the sum, and this revised addition is given under heading (2) in the last column.

9. From either (1) or (2) we are led to reject the suggestion of a periodicity near 30 days. It is chiefly due to the 1917–18 observations, and is not supported by the later ones. But perhaps a reservation may be made. If there was a physical change in the star in 1918, as indicated by the considerable reduction in fluctuation, there may have been a periodicity near 30 days before 1918. For elucidating this point we require earlier observations.

10. Divisor 32 gives the most conspicuous suggestion in either column (1) or (2). Recurring to the coefficients of $\sin\theta$ and $\cos\theta$, we get for the three determinations—

in 1917–18 . .	$0\cdot33\cos(\theta - 304^\circ)$
in 1918–19 . .	$0\cdot14\cos(\theta - 4^\circ)$
in 1919–20 . .	$0\cdot24\cos(\theta - 140^\circ)$.

If it is a real periodicity the phase should change uniformly; and since the intervals between consecutive sets are approximately equal (one year each), the mean of the first and third phases should agree with that of the middle. The difference 38° is not large, and we may accept this periodicity as possibly real. Since the phase increases 196° in two years, the period is longer than 6·25 days: we may put its length at 6·279 days, which is nearly 69/11 days. If we divide each cycle into 6 equal parts of length 69/66 days, the mean magnitudes for these parts are found to be—

	(1).	(2).	(3).	(4).	(5).	(6).
1916–18	13·62	13·43	14·04	13·78	14·02	13·56
1918–19	13·67	13·95	13·70	13·91	13·80	13·83
1919–20	13·11	13·02	13·26	13·40	13·53	13·11
Mean	13·47	13·47	13·67	13·70	13·78	13·50

The second group is not very accordant, but we are already prepared for this. The maximum and minimum may be put in columns (1) and (4) respectively, and to this test the second line responds: but it is anomalous in intermediate columns.

11. Turning now to divisor 28, period near 7·1 days, the results for the three years are—

$$\begin{aligned} 1917\text{–}18 \quad &. \quad . \quad 0{\cdot}12 \cos(\theta - 111^\circ) \\ 1918\text{–}19 \quad &. \quad . \quad 0{\cdot}10 \cos(\theta - 303^\circ) \\ 1919\text{–}20 \quad &. \quad . \quad 0{\cdot}11 \cos(\theta - 215^\circ). \end{aligned}$$

Now the mean of first and third phases is, as they stand, 163°, differing from 303° by 140°: but we may, of course, increase or diminish any phase by 360°. The phases would then be—

either	471°	303°	215°,	period 7·100 days
or	111°	303°	575°,	period 7·222 days.

We should normally expect to be able to choose the right alternative by inspection of the results for neighbouring divisors. Thus even in the value of R^2, if the true period is longer than 7·143 days (divisor 28), we should expect the results for divisor 27 to be larger than those of 20 in Table II. But the results for 27 and 29 are the same in column (2) and nearly the same in column (1). Even when we go back to the coefficients of $\sin\theta$ and $\cos\theta$, which ought to change sign at a periodicity, we get no help. This throws grave doubt on the reality of this term, doubt which is increased by the smallness of the coefficients.

12. With some diffidence I venture to ascribe this apparent periodicity to the effects of observing on regular days of the week. If we take the 7·100 days alternative and arrange 71 days in 10 sets of 7, we find the records all occurring in three boxes out of the 7, the other days being blank. When we proceed to the next set of 71 days, the occupied boxes change their position by one day, of course: so that we ultimately get material in every box. But that in one box is of very different date from that in another. Now the mean magnitude of Nova Persei is very different at different dates. Thus for 1917–18 December and January it is well over 14·00, while in 1919 October it is under 13·00. Hence we are liable to get a spurious periodicity from this lack of uniformity in date. We may collect the results as before—

	(1).	(2).	(3).	(4).	(5).	(6).	(7).
1916–18	14·22	13·55	14·00	13·25	13·74	13·53	13·65
1918–19	13·76	14·18	13·59	13·70	13·46	13·90	13·69
1919–20	13·40	13·40	12·93	13·25	13·14	13·02	13·58
Mean	13·79	13·71	13·51	13·40	13·45	13·48	13·64

But although the mean values show a distinct fluctuation, the variation from one group to the next is considerable, and almost certainly due to the cause above mentioned. A periodicity near

7 days should be very clearly marked to be above suspicion of this kind.

13. The shorter periods attracted first attention, because there are undoubtedly considerable fluctuations within a few days. But unless the 69/11-day period is real, there does not seem much chance of any *permanent* short-period term. We may now turn to the longer periods, and we are at once struck by a period of about 400 days. If we take means for consecutive sets of 40 days each, counting from J.D. 2421200, and arrange them in ten boxes, we get the following results :—

TABLE III.

Long Period (400 Days) in Nova Persei.

(1).	(2).	(3).	(4).	(5).	(6).	(7).	(8).	(9).	(10).
14·03	13·17	13·55	...	...	...	12·96	13·50	13·66	14·25
14·25	14·08	...	...	...	13·47	13·82	13·51	13·39	14·10
14·12	14·12	...	...	...	13·16	12·97	13·29	13·19	13·43
13·75	13·32	...	...	13·40	12·99	13·36	13·59	13·30	13·06
14·04	13·67	13·55	...	13·40	13·21	13·28	13·47	13·38	13·71

(The last six results are from unpublished observations by Barnard.) There is a clear and rather sharp minimum, but unfortunately the maximum has not yet been fully observed owing to the period being so nearly one year. The harmonics of this periodicity are probably noticeable, and we may perhaps ascribe the large value of R^2 for divisor 4 (period 50 days) in Table II. to this cause.

14. The period of 400 days corresponds to divisor 3 for the whole available range of 1200 days. If we now analyse the whole series for long-period terms (taking means for 10 days to shorten the work), we get results as in Table IV. We can interpret them in terms of the long-period term and its harmonics alone on the assumption that the true period is rather longer than 400 days. Thus the fourth harmonic appears, not with divisor 12, or period

TABLE IV.

Possible Long-period Terms in Nova Persei.

Divisor.	Period.	R^2.	Divisor.	Period.	R^2.
	d			d	
3	400	180	12	100	7
4	300	4	13	92	9
5	240	33	14	86	36
6	200	117	15	80	2
7	171	8	16	75	6
8	150	11	17	71	6
9	133	38	18	67	18
10	120	1	19	63	3
11	109	50	20	60	20

100 days, as we might expect, but with divisor 11, period 109 days. Hence the true period would seem to be longer than 400 days, though not much longer. Since R^2 is greater for divisor 6 than for divisor 5, the half period should be nearer 200 days than 240; *i.e.* less than 220 days. By similar inspection of successive harmonics we can thus assign as superior limits 440, 423, 460, 445, 414 days, etc. The guidance necessarily becomes weaker as we descend the scale: if we may trust that of the sixth harmonic the period must be <414 days.

15. After the paper had been sent in, some more observations were received from Barnard (and have now been incorporated in Table III.). For comparison with Table III. let us adopt a period of 420 days, which is about the longest likely to be consistent with Barnard's observations.

TABLE V.

Suggested Period of 420 *Days in Twelve Groups of* 35 *Days each, beginning with* 2421200.

(The unit is 0·01 mag., counted from 12·00. Suffixes show the number of nights of observation.)

(1).	(2).	(3).	(4).	(5).	(6).	(7).	(8).	(9).	(10).	(11).	(12).
203_5	117_3	184_2	97_1	...	...	...	96_5	150_6	166_{10}	201_5	222_5
228_3	...	...	...	...	...	171_7	192_6	136_8	155_3	215_9	202_5
211_2	...	...	...	93_1	106_9	104_9	137_5	133_7	163_3	147_4	...
...	...	...	140_6	98_9	131_8	152_5	167_4	109_3	...	...	...
212	117	184	134	97	118	138	139	135	163	197	212
189	173	153	132	117	113	119	135	155	176	191	195

16. Harmonic analysis of the mean curve shown just below the rule gives (in ordinary units)

$$13\cdot54 + 0\cdot42\cos(\theta - 328^\circ) + 0\cdot10\cos(2\theta - 315^\circ) + 0\cdot18\cos(3\theta - 229^\circ);$$

or, reducing to standard form,

$$2\cdot94\cos(\theta - 180^\circ) + 0\cdot70\cos(2\theta - 19^\circ) + 1\cdot26\cos(3\theta - 145^\circ).$$

The relatively large third harmonic makes it doubtful whether the long-period variable classification is relevant, but the phase of the second harmonic indicates Group I. The curve corresponding to the first formula is shown in the last line of Table V.

17. Using this last line to test the separate cycles for epoch, they give no clear indication of progression in phase, and we infer that Barnard's observations alone do not suffice to assign the period more closely than as 400–420 days. We may put his

max. at $2421813 \pm 420n$ to $400n$
min. at $2422023 \pm 420n$ to $400n$.

18. The question now arises whether we can trace the term in earlier observations. Looking backwards, we have

(*a*) A series of observations by Dr. W. H. Steavenson in the autumn of 1915 (*B.A.A. Journal*, 1916 January). They extend over 77 days only, and do not show much change in mean value, though they show fluctuations of short period. They do not help much for the present purpose.

(*b*) C. R. D'Esterre gives some photographic observations in *M.N.*, **74**, 535. Let us try the suggested period of 420 days: beginning with $2421200 - 5 \times 420 = 2419100$, and arranging them in twelve portions of 35 days as in Table V., we get the mean result of Table VI.

TABLE VI.

D'Esterre's Observations in $420 = 12 \times 35$ *Days.*

(Unit 0·10 mag.)

(2).	(3).	(4).	(5).	(6).	(7).	(8).	(9).	(10)
...	...	...	...	124_1	...	...	118_1	...
...	...	123_1	...	124_2	...	122_8	119_3	118_4
127_2	124_3	126_2	120_1	120_2	125_5	127_9	130_2	...

19. There are no observations in portions (1), (11), and (12), which have accordingly been omitted from the table. There would appear to be a distinct maximum about the end of the second line, with which the line above accords, but not the line below. Several reasons may partly explain the discordance. Firstly, the star was fading, the mean magnitude being about 12·3, whereas Barnard's observations show that a year later it was about 13·3; secondly, there are undoubtedly fluctuations in brightness of short period which may cause accidental deviations; thirdly, we may not have adopted the right period.

20. The last point is the most important for us: we can see that there is another apparent maximum in the third line about portions (5) and (6). The interval between the two, however, cannot be greater than nine portions of 35 days = 315 days, which is not reconcilable with what we already know of the period. If the period has remained constant (which is of course an assumption), one or other of these apparent maxima must be spurious. Their approximate dates are 2419835 and 2420115. Combining the former with Barnard's 2421813, we get 1978 days $= 5 \times 396 = 4 \times 494$ days; combining the latter, we get 1698 days $= 4 \times 425$ days.

21. Going further back, we find observations by Nijland (in *A.N.*, 4303) from 2416681 to 2418037. The mean magnitude was then 10·5, and the observer comments on the smallness of any deviations from it. Carrying back the period of 420 days, we get for the twelve portions

(1).	(2).	(3).	(4).	(5).	(6).	(7).	(8).	(9).	(10).	(11).	(12).
57	63	56	58	59	56	41	64	53	60	66	59

22. These figures give, in units of 0·01 mag., the excess over 10·00 mag. Harmonic analysis of them gives, in ordinary units,

$$10{\cdot}58 + 0{\cdot}04\cos(\theta - 340^\circ) + 0{\cdot}03\cos(2\theta - 195^\circ).$$

The phase of θ is not far from Barnard's 328°, but the coefficient 0·04 is only one-tenth of his coefficient 0·42. There may, however, be a sufficient reason for this discrepancy, without supposing any essential change in the 400-day variation. We have seen that the star was 3 mag. brighter when Nijland observed it in 1904–8 than it is now. If we may regard this difference as due to a quantity of added light which is independent of the 400-day term we are studying, the fluctuation will naturally form a smaller percentage of the whole. Thus let the light at minimum now be called L: then at maximum, which is 0·84 mag. brighter, the light is 2·2 L (since log 2·2 = 0·4 × 0·84). Hence the fluctuation represents an addition of 1·2 L. Now in Nijland's time the star was about 3 mag. brighter, and thus the light at minimum was 16 L: the addition of the same 1·2 L would only increase it to 17·2 L, representing a change in magnitude of (1·236 − 1·204)/0·4 = 0·08, which is just what we have found. If then we can trust so small a coefficient and the phase accompanying it, a period of 420 days will carry back to Nijland's time. But of course it is not the only alternative. The maximum may be put at 2417200, which differs from Barnard's 2421813 by 4613 days = 11 × 419 = 12 × 384 days.

23. In view of the great diminution of the coefficient (whether the reason be rightly assigned or no), it seems unprofitable to consider previous observations back to the time of the outburst in 1901. But before that the star was of about the same brightness as Barnard now finds it, and in *H.C.*, 66, Professor E. C. Pickering collected a number of observations of it derived from early photographs, as shown in Table VII.

TABLE VII.

Observations of Nova Persei before 1901 (*H.C.*, 66).

Date.	J.D. 2410000+	Obsd. Mag.	Phase of 396 Days. d	Barnard's Curve.	O−C.
1890 Oct. 26	1667	12·95	367	13·63	−0·7
Dec. 2	1704	13·37	8	14·00	−0·6
1891 Jan. 20	1753	<13·7	57	13·80	−0·1+
Mar. 11	1803	<13·4	107	13·28	+0·1+
Dec. 10	2079	<13·7	383	13·77	−0·1+
1893 Jan. 25	2489	14·06	1	13·91	+0·1
1894 Oct. 11	3113	13·15	229	13·23	−0·1
1897 Oct. 17	4215	13·46	143	13·40	+0·1
1900 Mar. 7	5086	13·36	222	13·29	+0·1

The first three columns are from *H.C.*, 66. The last three will be explained in a moment.

24. It is risky to make inferences from such isolated observations, but we may perhaps conjecture that the variation was more regular before 1901 than after, being free from the short-period fluctuations which resulted from the outburst of 1901. If so, there appears to be a minimum near 2412489. Adopting it as the exact date of minimum, as the best we can do, and comparing with Barnard's 2422023, we get a difference of $9534 = 22 \times 433 = 23 \times 415 = 24 \times 397$.

25. The last of these, 397 days, is close to the value which suits D'Esterre's observations on the more probable of two suppositions, while 415 days does not agree with the alternative. Of course the whole supposition of a permanent periodicity is precarious. There seems, however, to be something to be said for it: if so, 396 days seems the best value of the period. The light curve must be nearly as in Table III. We can only obtain an approximate light curve, owing to the paucity of observations in portions (3), (4), and (5). The figures of Table III. give

$$13{\cdot}52 + 0{\cdot}28 \cos(\theta - 8^\circ) + 0{\cdot}08 \cos(2\theta - 31^\circ) + 0{\cdot}12 \cos(3\theta - 352^\circ).$$

26. If we go back to the original observations and arrange them in 12 groups of 33 days each, we get the numbers of Table VIII.

TABLE VIII.

Barnard's Observations in Period of $396 = 12 \times 33$ *Days.*

(Unit 0·01 mag. above 12·00.)

(1).	(2).	(3).	(4).	(5).	(6).	(7).	(8).	(9).	(10).	(11).	(12).
203_5	109_1	167_3	97_1	...	...	...	178_1	76_4	149_8	170_9	215_5
224_5	218_2	...	...	...	...	...	163_5	188_7	138_5	142_6	211_7
211_6	212_4	...	...	...	...	98_2	106_8	112_7	137_5	133_7	146_2
156_5	...	...	...	...	152_2	129_{10}	90_8	151_4	159_7	184_1	100_5

O	199	188	167	97	(157)	152	124	117	137	147	153	176
C	202	190	144	128	143	150	130	120	131	144	151	176

We may either take the simple mean of the results, or the weighted mean. Since the deviations are of short period, the latter seems the more appropriate, but the other is nearly the same. Harmonic analysis gives

$$13{\cdot}51 + 0{\cdot}28 \cos(\theta - 4^\circ) + 0{\cdot}15 \cos(2\theta - 346^\circ) + 0{\cdot}13 \cos(3\theta - 51^\circ),$$

the corresponding curve being shown in the last line (C).

27. D'Esterre's observations, arranged in the corresponding groups, give in the mean (unit 0·01 mag. in excess of 10·00)

(1).	(2).	(3).	(4).	(5).	(6).	(7).	(8).	(9).	(12).
240_4	260_1	236_6	200_1	215_6	249_8	256_{10}	170_1	240_1	265_2

If we supply values for the missing groups (10) and (11) by interpolation, and analyse, we get

$$12{\cdot}38 + 0{\cdot}18\cos(\theta - 334^\circ) + 0{\cdot}14\cos(2\theta - 305^\circ) + 0{\cdot}22\cos(3\theta - 114^\circ).$$

28. Nijland's observations give sensibly zero for the coefficients, and thus contribute nothing to the evidence: but a possible reason for this has already been indicated. Perhaps the relations of the three sets of observations may be usefully indicated thus: let us arrange the groups according to the value of C in Table VIII., *i.e.* according to the magnitude as shown by Barnard's curve.

Table IX.

Group.	Barnard.	D'Esterre.	Nijland.
(1)	202	240	59
(2)	190	260	60
(12)	176	265	46
(11)	151	...	64
(6)	150	249	64
(10)	144	...	60
(3)	144	236	67
(5)	143	215	52
(9)	131	240	53
(7)	130	256	60
(4)	128	200	...
(8)	120	170	53

29. The correlation between Barnard and D'Esterre is 0·7: that between Barnard and Nijland is reduced to zero by the single discordant 46 for group (12) in Nijland due to two observations giving the star exceptionally bright. There are undoubted fluctuations of short period which may easily account for this, but they leave the whole question of *permanent* variability in 396 days uncertain.

30. The early observations of *H.C.*, 66, are compared with Barnard's curve in the last three columns of Table VII. The earliest two do not fit, but the others are in fair accord. It must, of course, be remembered that we selected the period to fit the observation of 1893 January 25, but there are six others which might easily have shown discrepancies and do not. It may be added that the observations of 1890 are described in *H.C.*, 66, as "somewhat uncertain."

Summary.

1. Barnard's observations of Nova Ophiuchi No. 2 (Hind, 1848) from J.D. 2422080 to 2422551 fit well with a period of 27·3 days. There is also possibly a periodicity of double this length.

2. His observations of Nova Persei No. 2 (Anderson, 1901) from 1916–20 have been analysed for periods from 400 days to 5 days. There is apparently a well-marked periodicity of something under 400 days (say 396 days), the harmonics of which are in good evidence. Beyond this there is nothing likely to be real except perhaps a fluctuation in 69/11 days. Another small fluctuation near 7 days is probably due to the interaction of the long-period term with the effect of regular weekly observing.

3. The long-period term of 396 days can be probably traced back to before the outburst of 1901, as shown by the observations in *H.C.*, 66. But *after* the outburst it was so much diluted by the flood of extra light that it is only doubtfully traceable until about 1914, when D'Esterre made some observations.

The Ultra-Violet Spectrum of Nova Aquilæ, 1918 June 10.
By the Rev. A. L. Cortie, S.J.

In my paper on "The Earlier Spectrum of Nova Aquilæ, 1918,"* the fiducial points for the measurements of wave-lengths were taken at the more refrangible edges of the bright bands at H_β, H_γ, and H_δ. These positions were in good agreement with the micrometer readings of a normal spectrum. The deduced wave-lengths of the dark absorption spectrum, and of the bright bands, were sufficiently accurate for the purpose of identification with the α Cygni spectrum. Thereby the main object of the paper was fulfilled.

In these earlier spectra of the nova the hydrogen series is double, the first series of lines being broad but well defined, while the second or more refrangible series is broad, fuzzy, and ill defined. In most cases, too, notably in H_β, H_γ, H_δ, the lines of the second series are composed of several thin bright lines. On account of the unequal displacement to the violet of these two series of broad lines, and also of the irrationality in the dispersion of a prismatic spectrum, in the more refrangible regions, overlapping of the two series of displaced lines takes place. Broad lines or narrow bands are accordingly formed which may also possibly contain coincident metallic lines, other than those due to hydrogen. The overlapping begins at H_μ of the first series, which forms a broad line with H_λ of the second series.

The purpose of the present paper is to separate the two series of lines in the ultra-violet, which was not successfully accomplished in my first paper. This was due to the inexactness in the selection of the fiducial points. Plates taken with a prismatic camera labour under the disadvantage, which is absent from photographs secured with slit spectroscopes, of the absence of a comparison spectrum.

* *Monthly Notices*, 79, 123.

But in the present instance two excellent fiducial points are furnished by the two narrow absorption lines of calcium at H and K. These are at their normal positions, and have practically no displacement in the line of sight.* The plate now measured was the first taken here, 1918 June 10, 11.0 to 11.30 p.m. G.M.T. In the most refrangible positions of the spectrum, additional check measurements have also been made on the second plate secured, June 10, 11.35 to 12.15 p.m. G.M.T. The photographs were taken with the Thorp prismatic camera.

Measurements have been made only as far as H and K, as in the less refrangible part of the spectrum the accurate measurements of Dr. Lunt, of Mr. Evershed, and of other observers, are amply sufficient. But beyond H and K very few wave-length measures have so far been published.

Adopting H_0 and K_0 as fiducial points, the normal measure of H_β falls exactly on a bright radiation line, which is coincident with the wide H_β radiation band. This line is in the middle of the band. The wave-length of K_0 being 3933·8, bright K_1 falls at 3914·5 and bright K_2 at 3904·0. These wave-lengths correspond to differences from K_0 of 19·3 and of 29·8 tenth-metres respectively, giving line-of-sight velocities of 1449 and 2288 km. per second.

Adopting these values, and using whole numbers, the displacements at H_ζ would be 19 and 30 units; at H_η, 19 and 29 units; at H_θ, 19 and 29 units; and in general at λ3800, 18 and 29; at λ3700, 18 and 28; and at λ3600, 17 and 27 units. As the following table shows, the computed wave-lengths of the two series of dark lines are in sufficiently good agreement with their normal wave-lengths for the purposes of identification. On June 10 each of the hydrogen doubles was accompanied by a broad bright band on the less refrangible side. Those at H_γ and H_δ were especially marked by their complicated structure.

The following table extends from λ3625 to H_0, λ3969, and comprises two series of computed wave-lengths, including the hydrogen series from H_ϵ to H_ω.

TABLE.

Measured W.L.	Intensity.	Series I. Computed W.L.	Series I. Probable Origin.	Series II. Computed W.L.	Series II. Probable Origin.	Remarks.
3608	1	3625	25 Ti			A series of broad lines or narrow bands.
13	2	30	31 Sc			
13	2	30	31 Sc			
16		33	33 Y	3643	43 Sc	
22	2	39	40 Pb			
27		44	43 Sc			
29	2	46	46 Sc			
36		53	52 Sc			

* Lunt, *Monthly Notices*, 79, 420; Evershed, 79, 478.

TABLE—*continued.*

Measured W.L.	Intensity.	Series I. Computed W.L.	Series I. Probable Origin.	Series II. Computed W.L.	Series II. Probable Origin.	Remarks.
3643		3660	60 Ti	3670	68 H_ω 70 H_ψ	H_ω series II. probably at this edge.
48	3	65	65 Y	75	72 H_χ 74 H_ϕ	H_χ II. in this blend.
53		70	68 H_ω 70 H_ψ	80	76 H_υ 79 H_τ	H_υ II. in this blend.
56		73	72 H_χ 74 H_ϕ	84	83 H_σ	
61	5	78	76 H_u	89	87 H_ρ	H_υ I. in this band.
62		79	79 H_τ 80 Ti			
65		82	83 H_σ	93	92 H_π	
67	7	84	85 Ti			
70		87	87 H_ρ	98	97 H_0	
75	3	92	92 H_π	3703	04 H_ξ	
80	3	97	97 H_0			
87	6	3704	04 H_ξ	15	12 H_ν	
95	5	12	12 H_ν	23	22 H_μ	
3703		20	20 Fe			Overlapping of H lines begins with H_μ I., which forms a broad line with H_λ II.
04	7	21	22 H_μ	32	34 H_λ	
08	7	25	25 Ti	36		
10		27	28 V. Fe	39	38 Fe ?	An arc line of Fe.
17	6	34	34 H_λ			
22	5	39	38 Fe ?	50	50 H_κ	
30	5	48	48 Y 50 H_κ			
38	7	55	55 Co			A broad thick line unlike that in α Cygni. Blend probably of H_ι II. and Ti lines 58 and 61.
46		63	62 Ti	74	71 H_ι	
53	5	71	71 H_ι			
69	5			98	98 H_θ	
80	6	98	98 H_θ			
87	2	3805	06 Fe			Weak in α Cygni and the Nova; strong in Procyon.
95	7	13	14 Ti			
3807	8	25	26 Fe	3836	36 H_η	
19	8	37	36 H_η			Mg 38, weak in α Cygni.
29	1	47	46 Co			Co 46, feeble in α Cygni.
37	4	55	55 Si	66	66 Fe	55, Intensity 5, α Cygni.
42	2	60	63 Si ?			63, Intensity 6, α Cygni.
47	4	65	66 Fe			66, Intensity 3, α Cygni.

TABLE—*continued.*

Measured W.L.	Intensity.	Series I. Computed W.L.	Series I. Probable Origin.	Series II. Computed W.L.	Series II. Probable Origin.	Remarks.
3853	2	3872	73 Fe			72, Intensity 3, α Cygni.
60	8	79	79 V	3890	89 $H\zeta$	79, Intensity 4, α Cygni.
70	8	89	89 $H\zeta$			
81	5	3900	01 Ti			01, Intensity 5, α Cygni.
86	5	05	06 Si			06, Intensity 4, α Cygni.
89	9	08	08 Sc			
94	6	13	14 Ti			13, Intensity 5, α Cygni.
3904	7	23	23 Fe	3934	34 K Ca	K II., a hazy broad line.
15	10	34	34 K Ca			K I., broad line.
29	3	48	49 Ti			A close triple forming a hazy blend.
31	3	50	51 Y			
33	3	52	52 Y			
34	3					Undisplaced K_0 fine line; fiducial point.
41	7			70	70 $H\epsilon$	And H II. Ca.
52	10	70	70 $H\epsilon$			And H I. Ca.
69	4					Undisplaced H_0 fine line; fiducial point.

In addition to the hydrogen lines, the other absorption lines are due to enhanced lines of iron, titanium, scandium, silicon, yttrium, cobalt, vanadium, and probably one line of lead. One arc line of iron, λ3738, is possibly present. Besides the Ca lines, H and K, it is probable that the lines 3643 Sc, 3738 Fe, and 3866 Fe are represented in both series. Otherwise the dark metallic lines are all displaced concomitantly with the first series of hydrogen lines.

This dark-line spectrum is essentially a spectrum of the type of α Cygni, even in the ultra-violet. We must, however, notice that the K line in the nova is very strong, while in α Cygni it is much weaker than the companion line. In this particular the nova's spectrum assimilates to that of Procyon. Again, in α Cygni the H_α line is very weak, while in the nova it is by far the strongest of all the hydrogen lines.

Summary.

1. The ultra-violet spectrum of Nova Aquilæ, 1918 June 10, is resolved into two series of absorption lines, including the hydrogen lines from H_ϵ to H_ω.

2. The spectrum is mainly of the type of that of α Cygni.

3. The metallic lines are displaced concomitantly with the first series of hydrogen lines.

Stonyhurst College Observatory:
1921 *April* 2.

Cepheid Variation: acknowledging a Correction and some Further Considerations. By C. D. Perrine.

In the *Monthly Notices* for 1920 March (p. 498), Professor Plummer, in his article on "Short-Period Variation," points out an erroneous conclusion as to the residual radial velocities of these stars on a pulsation hypothesis in my second paper on "Cepheid Variation" and a subsequent note.* I desire to acknowledge the justice of that criticism. This error arose from the general acceptance of pulsations as satisfying the radial velocities. This apparent confusion is discussed in a later paragraph.

Withdrawing the evidence of the residual velocities leaves no decisive test perhaps of duplicity. Yet it seems to me that duplicity is favoured apart from Professor Plummer's objection to the pulsation hypothesis, viz. that it lacks a known source of energy to maintain the pulsations. Indeed, to me such an objection seems of little weight in the face of the evidence which is accumulating of the existence of cosmical matter in the galactic regions.

The importance of throwing all possible light on Cepheid variation, and especially the question of duplicity, impels me to add some further comments and considerations to the discussion.

There are several points of view in Professor Plummer's valuable and stimulating paper with which I cannot quite agree. It is not my intention to discuss such divergences here, as they are largely at present matters of personal preference. There is one point, however, which I would call in question. Professor Plummer discusses the variable-star question in general and the evolution of double stars (with regard to their spectra) from the standpoint of internal or self-contained conditions almost wholly.† The galactic preferences of many objects, including some of these, together with the evidence of cosmical matter in those regions which is accumulating, seem to me to point in no uncertain manner to the possibility of an external cause capable of producing some of these phenomena, and to the further possibility that the chief part may be of the surface only. Cosmical matter and gravitation also furnish a possible source of energy to satisfy the wastage of stellar radiation demanded by recent investigations without recourse to the liberation of subatomic forces.

1. Regarding the fidelity with which the velocity curves of Cepheids are represented by orbital motion, I feel that my statement on that point has been somewhat misconstrued. With full knowledge of the discordances which existed, my opinion of their weight is very well expressed by Professor Plummer when he says: "It may be that the anomalies need not be taken too seriously." I therefore purposely used the word "satisfactorily" in place of a stronger one. When the general agreement of the observed

* *Astrophysical Journal*, 50, 81, 148, 1919.
† *Monthly Notices*, 80, 506 *et seq.*, 1920.

velocities with orbital motion is noted, and in the absence of stronger evidence to the contrary than seems now to be forthcoming, it seems to me that the representation, even if not perfect, is satisfactory. When the line displacements of the Cepheids were discovered, and for some time afterward, were they not accepted unhesitatingly as satisfactory proof of orbital motion? Indeed, Professor Plummer in 1913 expressed himself as follows*:

> "We know that variables of the δ Cephei type, when submitted to the test, have invariably proved to be spectroscopic binaries."

There is no hint here that the velocity curves are not satisfactory.

2. In my second paper on "Cepheid Variation"† I stated that among the spectroscopic binaries there appeared to be a dependence of orbital eccentricity upon the masses of the components and their separations. The systems with the smaller secondaries showed in general larger eccentricities. There appeared also to be a dependence upon separation, probably the same that had been noted by other investigators as a dependence upon length of period.

A dependence upon period or separation in the short-period spectroscopic binaries is difficult to isolate from the existing data. The dependence upon relative mass appears to be more marked among these stars, with the exception of those of class B. The amount of data is not large, but appears to be sufficient sensibly to obliterate uncertainties of inclination and to give confidence in the reality of this dependence

Evidence has since been found of such a dependence among the visual binaries and in the solar system, where the smaller planets, satellites, and asteroids have, on the average, the more eccentric orbits. As the evidence in the solar system appears to be free from uncertainties, and that from the visual binaries but little less definite, it seems justifiable to conclude that they strengthen the evidence of such a dependence found among the spectroscopic binaries. A preliminary paper on the subject is in preparation.

As the Cepheids fall in line, in respect to such a dependence, with other spectroscopic binaries whose duplicity has not been questioned, we appear to have confirmatory evidence of the binary nature of the Cepheids. That pulsation phenomena would produce velocity curves agreeing so closely in form with those from orbital motion seems highly improbable.

3. It has been tacitly assumed that the observed agreement between the light and velocity curves satisfies the requirements of pulsations. This assumption is open to doubt, and does not appear

* *Monthly Notices*, 73, 658, 1913.
† *Astrophysical Journal*, 50, 86, 1919.

to have been sufficiently considered. Shapley explains the agreement thus: *

> "We may suppose that because of the internal vibration the photosphere of the star is periodically scattered or broken through by the rush of hotter gases from the interior. Maximum light and maximum velocity of approach would obviously be approximately synchronous. . . ."

Violent eruptions of gases from the interior are what, in my opinion, should occur in pulsations on the scale necessary to satisfy the radial-velocity variations. Something of the nature of pulsations of light occur in the Novæ some time after the outburst, accompanied by a violently disturbed spectrum. It seems impossible, however, to reconcile the quiescent spectra of the Cepheids with such violent activity as the hypothesis of pulsations demands. It does not seem to me that in such a case maximum light and maximum velocity of approach would necessarily be approximately synchronous, as Shapley infers. Would not the maximum of light occur near the maximum of *expansion*, which is *not* the maximum of velocity?

4. Messrs Martin and Plummer make what under the circumstances is perhaps a more tenable suggestion, as it contains the only unquestionable factor, change of size, which must exist according to the hypothesis, viz.: †

> "The factors mainly concerned may be taken to be the varying size of the star, the radiating power of the photosphere, and absorption in the outer layer."

No discussion is entered into as to the probable effects of these different factors. The important bearing which such effects may have upon the relations between the radial velocities and light variations appears not to have been pointed out.

The change of size required by the pulsation hypothesis is a very definite thing, whose amount and phases can be obtained with accuracy from the radial motions. If there were no other factors the size would furnish a crucial test of the pulsation hypothesis, as will be shown presently. The effects to be expected from the other factors are almost unknown, and may even be negligible.

The changes of size required by the pulsation hypothesis are very large. The semi-amplitude for fourteen Cepheids averages 1,280,000 kilometres. In other words, the average diametral distortion of these stars would have to be approximately twice the diameter of our sun. On the giant hypothesis the volumes of the larger ones would nearly double. If of the size of our sun at minimum, the volumes would be increased over twenty times at maximum.‡

* *Astrophysical Journal*, **40**, 464, 1914.

† *Monthly Notices*, **75**, 574, 1914–15.

‡ The spectral type of the Cepheids, on a pulsation hypothesis, is essentially the same at maximum as at minimum size. The deviations occur at intermediate phases. This is scarcely what would be expected from such changes of size.

It has been established that the maxima and minima of light occur at, or very near, the times of minima and maxima of the algebraic radial velocities, or the times of maximum velocity of approach and recession respectively of the primary upon the binary hypothesis. Upon the pulsation hypothesis (free oscillations) the maximum velocity of the wave must occur not at the maximum or minimum of expansion, but at points midway between. At the points of greatest expansion and contraction the radial velocity of the wave must be zero. Oscillations other than free will give different positions for maximum velocities, but at the greatest expansion and contraction the radial velocities in all cases must be zero.

If the light changes of the Cepheids are due to pulsations in which the effective radiation remains sensibly constant and the variations in amount of light result from differences of size only, the maxima and minima of light could not occur at the minima and maxima (algebraic) of radial velocities, as actually observed, but at points intermediate. The relation of the curves of radial velocity and expansions are well illustrated by curves A and B of Martin and Plummer * for RR Lyræ. A comparison of the displacement curve B with the light curves C and D shows clearly the lack of agreement necessary to satisfy the requirements of light and expansion.

If it were known that size was the only effective factor, this disagreement would be a crucial test of the theory of pulsations, and show that that theory was untenable. We do not know that the effective unit radiation is the same at the times which would have to correspond to maxima and minima of size, although the similar spectral types, in the absence of other evidence, tend to favour such a possibility. As nothing whatever is positively known about the changes of radiation (or other possible factors) to be expected in such pulsations, it is impossible to draw a conclusion further than this—that the disagreement of the light and displacement curves shows either

A. That other factors, whose effects as to amount and phase are entirely unknown, obliterate the effects of changing size; or

B. That the observed light variations are not due to pulsations.

To avoid misunderstanding, let me say that I do *not* consider size the only possible effective factor in a pulsation hypothesis, but the only factor about which anything definite can be assumed. To assume that such changes of size are negligible in producing changes of light is to reject what appears to be the only known and positive effect of such an hypothesis, and to ascribe essentially all of the light changes to effects or causes which may, and probably do, exist, but about the presence and behaviour of which nothing whatever is known in these stars. Such an assumption is, of course, not justified.

* *Monthly Notices*, 75, Plate 21, 572, 1915.

In the light of our present knowledge, therefore, it does not appear justifiable to conclude that the observed light changes satisfy the requirements of pulsations.

5. I have emphasised the importance of the strong preference of the Cepheids for the Milky Way. This preference is one of the most definite and apparently unencumbered facts connected with these stars. It is not simply accidental, and can no more be ignored than can a similar preference of the Novæ. Any hypothesis as to the cause of the variations in these stars to be tenable must take this preference into account. The failure to do so constitutes, in my opinion, a strong objection to any theory based solely upon orbital or geometric peculiarities, including the pulsation hypothesis, unless the pulsations are due to an external cause.

Professor Plummer has deduced a standard of comparison by which the galactic concentration of stars may be judged. In this connection it should be remembered that stars of the brightness of at least the brighter Cepheids and of spectral types F and G are, according to the Harvard observers,* more thickly scattered in the extra galactic than in the galactic regions. This fact tends still further to accentuate the galactic preference of the Cepheids.

In view of the preference of all early-type objects for the Milky Way, it would seem not inappropriate to seek in a common cause a similar preference of the Cepheids. I have already referred to the possibility that cosmical matter in some form, whose existence in those regions is fairly well established, may be the cause not only of early-type phenomena but of Cepheid variation as well.† There is a wide difference between the phenomena exhibited by the Cepheids and Novæ, for example, but it is conceivable that this may be of degree rather than of cause. At this time I would only refer to the effect on the spectrum of adding such matter. If slowly and uniformly added it might be expected to reduce the thickness of the absorbing ("reversing") layer of gases overlying the photosphere, tending toward the obliteration of some lines and the narrowing of most, raising the surface temperature, with a consequent moving of the energy maximum toward the violet. This appears to agree with the behaviour of the lines in the spectrum of δ Cephei as observed by Adams, who finds that the lines are wider at minimum than at maximum of light.‡

The general effect on the spectrum which has been observed would appear to be better explained by solid than by gaseous matter. If matter in the gaseous condition were added, selective action on a few lines rather than very general effects might be expected.

If such matter were added rapidly and in irregular amounts, a "silting up" of the spectrum might be expected, of which we may have an exaggerated case in the Novæ outbursts.

Variations in the amount of such added matter furnish a

* Twenty-second Meeting American Astronomical Society, 1918.
† *Astrophysical Journal*, **47**, 289, 1918; *ibid.*, **50**, 100, 1919.
‡ *The Observatory*, **42**, 167, 1919.

possible explanation of irregularities in the light curves, and might account also for irregularities in the displacements of the spectral lines.

6. A point to be borne in mind as having a possible bearing on the galactic preferences of the Cepheids and the hypothesis of an underlying external cause is the assumption that the cluster variables belong to the same class. This assumption of similarity rests chiefly upon the spectroscopic observation of RR Lyræ, which has a period of about the same length (one half day) as the cluster variables. It is to be noted, however, that RR Lyræ is not situated in a cluster. It appears to belong unquestionably to the Cepheid class, but whether the short-period variables in clusters show the characteristic displacement of spectral lines of the Cepheids is not known. Until that fact is established it is as well to keep in mind that there may be a distinction.

If the cluster variables are true Cepheids they also must be explained by whatever cause underlies the Cepheid activity. The fact that they are as restricted in their distribution is perhaps as significant as the preference of the galactic Cepheids in indicating an external cause for their variations.

7. There is one outstanding peculiarity of the orbits of the Cepheids which has been considered as a stumbling-block to a binary explanation. It is the preference of ω for 90°. It should be noted, however, that four stars (ζ Geminorum, Y Ophiuchi, Y Sagittarii, and SU Cygni) differ widely from 90°. Three of these stars are closer to 270° than to 90°. These four stars all have small orbital eccentricities. The distribution about 90° is not at all according to the law of errors, but marked preferences are shown for several restricted deviations, viz.:

7°, 5°, 4°, 5°
13°
24°, 20°, 20°, 21°, 21°
58°
112°, 117°, 104°

Whether or not these groupings are significant it is not possible to say.

The preferences for restricted values of ω itself are also more marked than called for by probability, although not so marked as the deviations.

No satisfactory explanation has been found for this preference of ω for 90°.

In the present state of our knowledge it would be unwise to insist too much on the peculiar preference of ω either favouring or opposing any particular hypothesis. It might prove to be analogous to a case which is known to have occurred of the discovery of a satellite with high orbital inclination in an extensive system whose orbital inclinations had all been previously very small.

It is perhaps not impossible that gravitational effects of the system may cause a preference for radial arrangements of the major axes of these orbits. There are indications that the preference for 90° is greater among the more eccentric orbits. Can it be that such an arrangement would be favoured by the small sizes indicated for the secondaries?

Summary.

1. The residual radial velocities of the Cepheids, as Professor Plummer points out, do not constitute a test of the pulsation hypothesis.

2. A source of energy to maintain pulsations or fluctuations may exist in the form of cosmical matter in the galactic regions.

3. Although the velocity curves of the Cepheids are not all perfectly represented by orbital motion, the deviations are not large, and, taking all things into account, the representation appears to be sufficiently good to satisfy an orbital explanation.

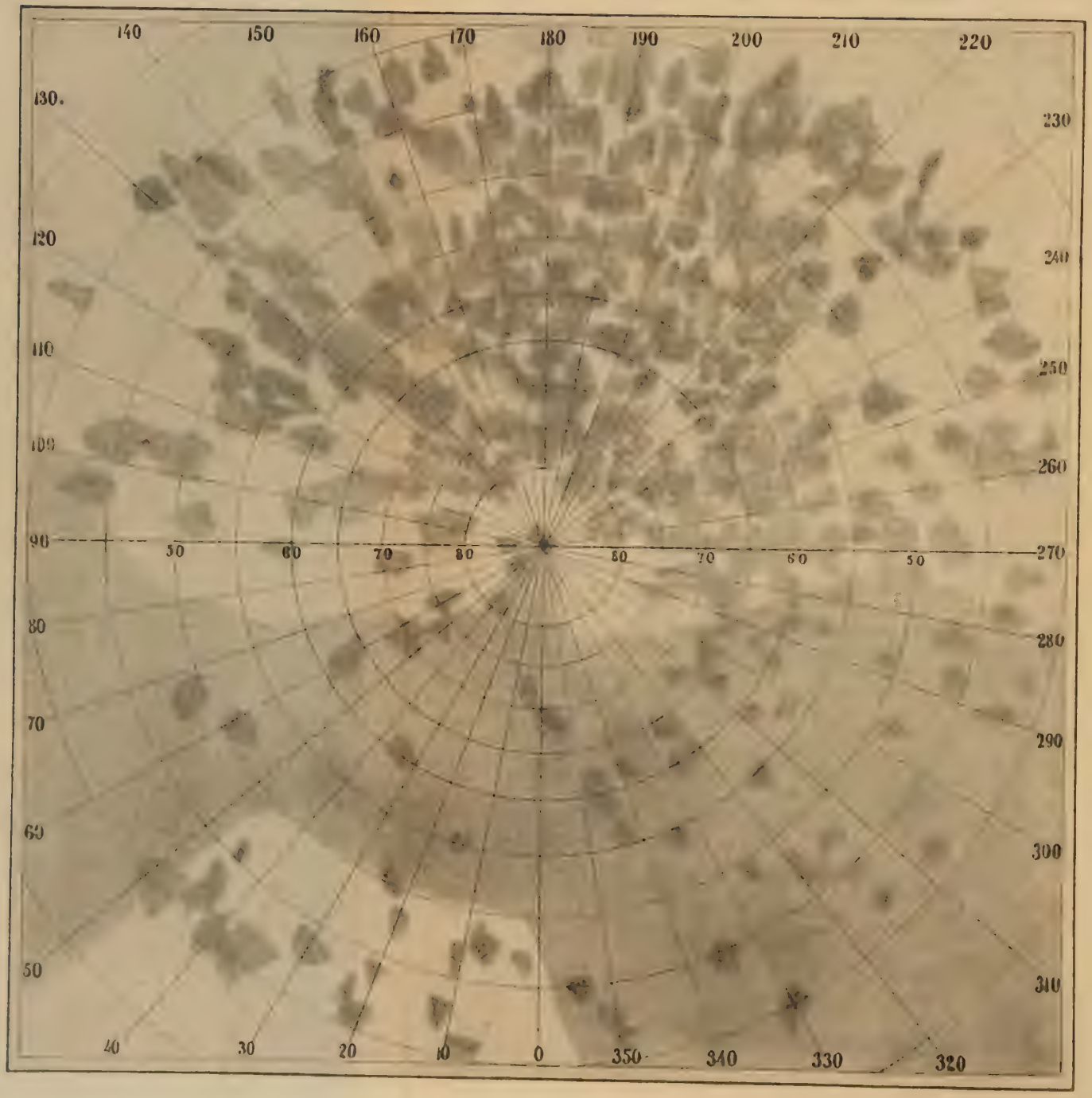
4. Evidence of a dependence of orbital eccentricity upon relative mass of the components has been found in the spectroscopic and visual binaries and in the solar system. As the Cepheids fall into line with other spectroscopic binaries in this respect, it is presumptive evidence favouring duplicity.

5. The phases of the light variations of the Cepheids are not in accordance with the changes of size demanded by pulsations, which are the only factor that it appears possible to predict with certainty on that hypothesis. If the Cepheid variations are in reality due to pulsations, the actual factors causing the light fluctuations and their behaviour are at present unknown.

6. On the hypothesis that cosmical matter is added to the star's atmosphere, it was predicted that the absorbing or reversing layer would be thinner at light maximum, a conclusion which appears to agree with the behaviour of the lines in the spectrum of δ Cephei.

7. If the cluster variables are true Cepheids, which is not yet fully established for radial velocities, their restricted preference as to distribution is not out of harmony with the hypothesis of an external cause as the principal factor in this type of variation.

Observatorio Nacional Argentino, Córdoba:
1920 *October* 23.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50

A Map showing Obscure Nebulæ and their Situation towards the Milky Way. By the Rev. J. G. Hagen, S.J., Assoc. R.A.S. (Plate 9.)

I. *Description of the Map.*—1. The smooth continuous streak represents the Milky Way in its true outlines and brightness, as determined by Heis and Gould. The design was facilitated by a table published in *Harvard Annals*, **48** (1903), p. 154.

The dark patches indicate cosmic clouds observed in Rome. The map is not the result of a systematic review of obscure nebulæ; it is rather a by-product of another plan of work, viz. the reobservation of the nebulæ collected in the *N.G.C.* of Dr. Dreyer. The main purpose of this programme was to estimate the brightness of these luminous objects, while the clearness or cloudiness of the surrounding portions of the heavens was noted down only as an accompanying remark.

The density of these obscurities was estimated by means of a scale of 5 grades, such as Heis used for mapping the Milky Way.

This method of reviewing the heavens explains why the map shows only discrete patches. A systematic *Durchmusterung* of cosmic clouds, which is now under way, will probably exhibit a continuous streak of obscurities, a "Nebular Way," as it might be called.

2. The centre of the map is the pole of rotation; the meridians reach down to 50° N.P.D. or 40° Dec. Thus far the reobservation of Dr. Dreyer's catalogue is complete, while the remaining portion of the sky, reaching down to −20° Dec., will be finished within a year.

The pole of the galaxy ($12^h\ 40^m$, $+27°$) lies outside the upper limit of the map. The drawing is not supposed to be artistic, though exact as to positions and shadings, both for the Milky Way and the cosmic clouds.

II. *Historic Notes.*—Obscure nebulæ were seen long before this, first by Sir William Herschel, who mentioned them in the *Phil. Trans.*, 1818.

Dreyer's catalogue contains a number of diffused nebulæ, thirteen of which have been identified as obscure clouds in the course of our programme.

In the *Atlas Stell. Var.* there are ten charts on which notice is given to the observer that the field is permanently nebulous.

Lately Professor Barnard published a list of 182 "dark markings" found on his photographs of the Milky Way [see *Ap. J.*, **49** (1919), p. 14]. The few nebulæ which Barnard saw directly in the heavens are undoubtedly identical in nature with those on the map. The greatest number, however, were collected from photographic plates, and these are difficult to identify at the telescope.

III. *The Results.*—Thus far two results seem to be established beyond doubt; the first one is directly visible on the map.

1. The entire sky outside the galactic plane is covered with dense cosmic clouds. Along the Milky Way the obscurities are

less dense and far between, although thin nebulous veils are found everywhere. An entirely clear sky is very seldom met with, and then only around stellar clusters. In fact, the more compact and brilliant a cluster, the clearer the background. The densest masses of nebulæ seem to group around the galactic pole and the region near Orion, although these portions of the heavens are not yet completely examined.

This first result contradicts the prevailing opinion that obscure nebulæ occur mainly in the Milky Way. The explanation is quite plain. Isolated clouds are easily recognised by contrast with the clear sky around, especially in the starry fields of the galaxy. But when the telescope is pointed towards the galactic pole, clouds are seen all around; they are then recognised only by the various shadings from one point of the field to another.

A year ago an astronomer friend was invited to look through the 16-inch refractor of the Vatican Observatory and to convince himself of the visibility of these obscurities. When he was told that he was looking straight at a nebulous field, he exclaimed: "Why, I have seen this before." Of course, *seen*, but not recognised. Probably many a record of "poor seeing" should be interpreted as a record of cosmic clouds.

2. The second result is that spiral nebulæ abound exactly where the cosmic clouds are most dense. This is curious enough, but it is not all: the bright nebulæ are found chiefly along the rim of extended clouds, while inside of large nebulous continents they are less frequent.

IV. *Conclusions* from these observations are perhaps premature, but may serve as guides for future research.

1. One overpowering impression which the cosmic clouds make on the observer is, that the diffused nebulæ which appear to us bright, like those in Orion and around the Pleiades, do not shine in their own light, but in the reflected light of neighbouring stars. The same holds for the many nebulous atmospheres in which a bright central nucleus is involved, except, of course, the planetary nebulæ.

This impression leads at once to the conjecture that the stars were formed from these clouds, and that the Milky Way has made a cavity in the surrounding ocean of primitive matter. The isolated nebulous veils which are still seen along the Milky Way are like the residues left over from the process of concentration.

2. This theory has the advantage of answering definitively the long-standing question, why the spirals seem "to avoid the Milky Way." Being associated, as we have seen, with the densest cosmic clouds, they surround the galactic cluster in all directions. If we assume the mean equatorial diameter of the galaxy, with Professor Charlier, six times as large as the polar diameter, and if we attribute the same dimensions to the hollow of cosmic matter in which the Milky Way is imbedded, the light coming to us from the spirals situated around the galactic equator will be thirty-six times less than that from the poles, and hence too faint to impress an image on the photographic plate.

3. On the other hand, the idea of a permanent cavity in which our galaxy should rest seems to encounter a serious difficulty. The radial velocities of about thirty spirals have been measured by Slipher and Pease, and have been found in some cases to be greater than 600 kilometres per second. Hence it would seem as if our stellar system could not remain in the hollow of cosmic matter resulting from its original formation.

Yet this relative motion between the spirals and our own system rather strengthens the theory. A preliminary computation by Young and Harper in the *Journal of the R.A.S. of Canada*, **10** (1916), p. 134, and by Truman in *Pop. Astr.*, **24** (1916), p. 111, puts the apex of this motion in the constellation of Capricornus, close to the Milky Way. As the motion is undoubtedly tangential to some orbit, it points to a rotation of the galaxy (or part of it) in its own equatorial plane. Similar rotations have been found in spiral nebulæ, like that of Andromeda.

It follows that a rotation of our Milky Way not only agrees with the theory of our stellar system being the result of concentration from previously existing cosmic matter, but that it is, in all probability, even a dynamical exigency of the process described.

Evidently this view of the universe favours the idea that the spiral nebulæ are cosmic formations outside and independent of our Milky Way.

Vatican Observatory:
1920 *October* 15.

Changes in the Lunar Craters Aristillus and Eratosthenes observed at the Harvard College Observatory, Mandeville, Jamaica. By Sir W. H. M. Christie, K.C.B., F.R.S. (Plates 10, 11.)

During a visit to Professor W. H. Pickering at Mandeville, Jamaica, last February, I was fortunate in having the opportunity to note with the 11-inch Draper refractor remarkable changes in the lunar craters Aristillus and Eratosthenes, and also in the Bradley Snow-Field, and thus to confirm Professor W. H. Pickering's observations made under similar conditions of illumination by the sun at corresponding co-longitudes.

I was able to observe the craters and the snow-field on Feb. 18, 21, 22, 23, and 26 (civil), 5–6 a.m., though the seeing was generally only 9 on a scale 1 to 12,* and overcast sky cut short the available observing time. This year the weather was quite exceptionally unfavourable during my visit to Jamaica, as, after prolonged drought, the rains with heavy thunder-showers and cloudy skies came in February and March instead of in May as usual.

* Seeing 9 would mean that the star disk is sharply defined but inner ring in constant motion.

Aristillus.—On Feb. 18 the "double canal" which starts within the crater and extends some distance outside was seen distinctly divided *inside* the crater and quite sharp throughout. The co-longitude was then 47° (9 p.m. Jamaica time, which is five hours earlier than Greenwich), the co-longitude for its noon being 89°, corresponding to its summer solstice. It is to be noted that for the moon the day and the year are the same, each being the lunation.

Professor Pickering describes the "double canal" at this co-longitude as starting at a point half way up the inner slope of the crater (which is 34½ miles in diameter), 5 miles within the rim where the "canals" are 0″·7 apart (centre to centre), and 0″·25 or 1500 feet wide, being of the same width as outside. Their length from origin to rim is 5 miles, from rim to bottom of outer slope 3 miles, and 3 miles more out, making 11 miles in all. Professor Pickering has also at this phase noted other dark streaks or so-called "canals" outside the crater, but my attention was concentrated on the double canal. The sky clouded over by 11 p.m.

On Feb. 21 I noted some changes in Aristillus, though the seeing was too variable for a sketch.

On Feb. 22, between 10 and 12 p.m., co-long. 96°, I noted remarkable developments outside the crater, and I made a sketch at the telescope of dark streaks and patches or "fields" which had appeared since Feb. 18. The "double canal" in particular had increased in width and spread into a dark patch on the north side, which was crossed by several broad dark streaks. There also appeared on the north side, outside the crater, a curious marking exactly like a short ladder with four treads, equally spaced. On the south-west side two dark streaks were noted.

By Feb. 23, 11–12 p.m., co-long. 109°, the dark streaks and patches seen the night before had still further developed and darkened, as shown by a sketch I made at the telescope, which I made quite independently of Professor Pickering, as I had by now grown sufficiently familiar with the crater at full moon to identify it without reference to him.

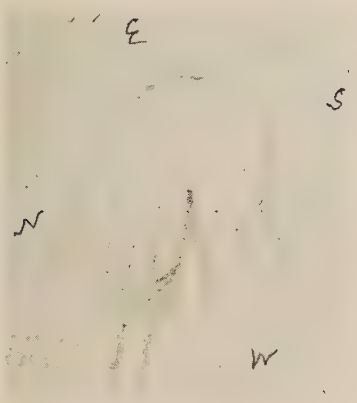
My next chance of observing Aristillus after a completely cloudy night on Feb. 24 was on Feb. 26, 5–6 a.m., civil reckoning, co-long. 136°, when the sky began to clear shortly before sunrise, after being persistently cloudy up to 5 a.m. The dark patches had then faded, and the dark streaks grown narrower. A striking change was noted in the "short ladder" on the north side of the crater, this marking having grown much narrower, while the four cross streaks had disappeared. It had also grown fainter. A sketch was made at the telescope of the appearance of the markings on this morning.

Eratosthenes.—This crater is in longitude 11° E. and latitude 14° N., so that the co-longitude for its noon is 101°, corresponding to its summer solstice. Its diameter is 37 miles, and the height of its walls 3 miles, the steepest slope of wall inside being less than 20°.

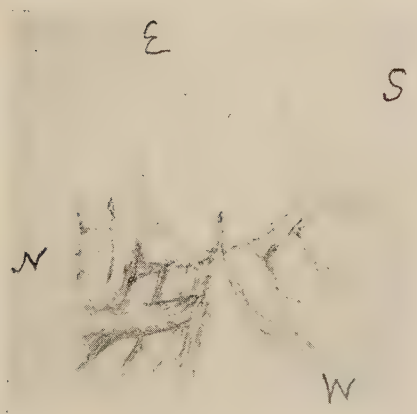
I observed it on Feb. 18, co-long. 47°, and Feb. 21, co-long. 84°, when Professor Pickering made rough sketches to enable me

ARISTILLUS.

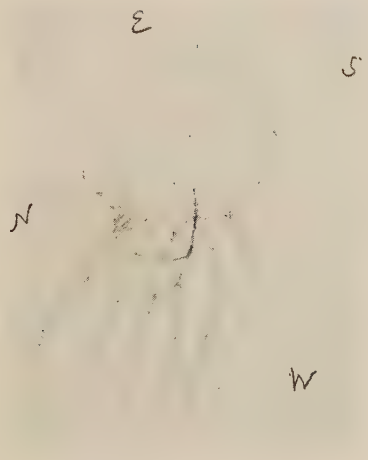
1921 Feb. 18, 11–12 p.m.
Co-long. 47°.



1921 Feb. 22, 11–12 p.m.
Co-long. 96°.



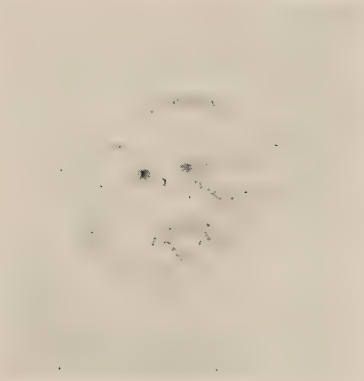
1921 Feb. 23, 11–12 p.m.
Co-long. 109°.



1921 Feb. 26, 5–6 a.m.
Co-long. 136°.

ERATOSTHENES.

1921 Feb. 22, 10–12 p.m.
Co-long. 96°.



1921 Feb. 23, 11–12 p.m.
Co-long. 109°.

1921 Feb. 26, 5–6 a.m.
Co-long. 136°.

to identify the crater; and on Feb. 22, 23, and 26 a.m., when I made sketches which are reproduced.

On Feb. 22, 10 p.m. to midnight, co-long. 96°, the dark markings inside the crater had developed, and in particular a V-shaped marking was seen inside the crater. I have not shown in my sketch any of the patches seen outside.

On Feb. 23, 11 p.m to midnight, co-long. 109°, the dark markings both inside and outside the crater had developed, as indicated in the sketch, which was made at a time when the sun was nearly vertical for the crater.

By Feb. 26, 5–6 a.m., co-long. 136°, my next chance of observing, there was a remarkable development in the dark markings, as my sketch made at the telescope, quite independently, indicates. On this morning, as on Feb. 23, Professor Pickering did not make any sketch to guide me. The dark streaks, with small round patches in their course, made a very complex formation, which I copied as carefully as I could; and in the outer streak, within the crater on the west, there was a portion where it was distinctly divided into two very narrow streaks. A comparison made *afterwards* with a much earlier drawing of Professor Pickering's, at the same longitude, shows a very remarkable agreement in *all* the details both inside and outside the crater, which gives striking evidence of the permanence of these markings under similar conditions of co-longitude, corresponding to similar seasons of the lunar year for the crater.

It is to be noted that the three bright spots, presumably snow-covered peaks, shown on the sketch on Feb. 23, were also seen on Feb. 22 and 26, though not put in the sketches on these days, for lack of time.

In some of the markings on the figures the slight differences of tint have been somewhat exaggerated in reproduction.

Changes were also noted in the mountain-ridge Bradley, which lies to the north-east of the small crater Conon, near the crest of the highest portion of the Apennines. On Feb. 21 and 22 a bright extension of the range to the south-west was noted, indicating a fresh fall of snow since Feb. 18, when Bradley was first examined; while on Feb. 23 the end of the ridge was seen separated as a round bright spot, which appeared more distinctly separated on the morning of Feb. 26, indicating a melting of the snow under the influence of the sun's rays after noon, which occurred for the crater between Feb. 21 and Feb. 22.

The reappearance of Saturn's ring after the passage of the earth from the dark to the bright side was noted on Feb. 22, when it was seen as a very fine bright line, of estimated breadth 1″·0. On Feb. 23 it was brighter, and its estimated breadth had increased to 2″·0. The ring was quite invisible, though carefully looked for on Feb. 21, 10–12 p.m.

Downe, Kent:
1921 *April* 8.

Observations of Level with the Transit-Circle of the Radcliffe Observatory, Oxford, during the Great Earthquake of 1920 *December* 16. By Dr. A. A. Rambaut, F.R.S., Radcliffe Observer.

During my absence from home Mr. Robinson, first assistant at the Radcliffe Observatory, informed me of an unusual phenomenon observed by him in the course of his observations with the Transit-Circle on the early afternoon of December 16, which puzzled him considerably at the time, but which was traced by him to the passage of earth tremors from the great Chinese earthquake of that date, reported in the papers on the following day.

With a view to putting these observations on record, I asked him to send me a detailed account of the phenomenon, which seemed to me of unusual interest. His letter, practically as it was written, follows :—

"*Radcliffe Observatory, Oxford.*
"1920 *December* 16.

"Dear Dr. Rambaut,—In accordance with your desire I will give a few details of the remarkable phenomenon observed with the Transit-Circle on December 16, which caused so much perplexity at the time.

"Requiring a new determination of level-error by the ordinary Nadir observations, I seized an opportunity, indicated by the steadiness of the magnetometer, when the engine at the University Press was at rest.

"The images of the R.A. wires reflected from the mercury were found to be perfectly sharp, but after making one setting, *i.e.* by placing the wires, direct and reflected, in close juxtaposition, I was astonished to see the reflected image slowly move away until it reached a distance of about 4″ : it then, as slowly, returned to the direct wire, repeating this many times during the half-hour's observation from $0^h\ 55^m$ p.m. to $1^h\ 25^m$ p.m. At times the amplitude of the oscillation was rather greater than 4″, possibly 5″.

"There was a complete absence of tremor, or quick vibration and diffusion, such as those ordinarily observed, caused at certain times by the Press engine, and occasionally by heavy traffic.

"Mr. Balk's observations corroborated mine. I remarked to him that I had never seen a similar phenomenon, and that its cause must be investigated. I also said it might be traced to some defect in the mercury-trough, or, which seemed unlikely, the axis of the instrument must be changing its level.

"At first I thought that possibly the oxidation of the mercury was sluggishly seeking equilibrium, or perhaps that the mercury itself was too shallow in the trough. But after cleaning the surface I found the same variations proceeding as before. Urgent duties forbade our making further tests at the time. We therefore proposed to experiment on the following day with another mercury vessel. This was, however, rendered unnecessary

by the announcement of the great earthquake disturbances at the time of our observations.

"Although we were thus prevented from making a satisfactory level observation, it is fortunate that two settings of the R.A. micrometer were recorded, for they enable us, with some later estimates, to state fairly definitely (1) that the amplitude of the oscillation in R.A. for both reflected and direct wires was 2″ or 2″·5; and (2) that this oscillation took place always in such a way as to *lift* the Eastern pivot relatively to the Western, the difference in height of the two pivots (50 inches apart) being altered, at the extreme oscillation, by the amount of about 0·0006 in.

"On the two succeeding days, December 17 and 18, at the same hour as before, the reflected wires were steady, and the micrometer readings of level (and collimation) very accordant.—Yours faithfully, W. H. Robinson.

"*Postscript.*—The two settings of the R.A. micrometer, referred to in the last paragraph but one, are

r
21·277
·333

"The corresponding readings on the two following days were

Dec. 17.	Dec. 18.
r	r
21·347	21·350
·358	·354
·360	·356
·357	

"One revolution of the micrometer = 31″·94."

This letter was communicated to Professor Turner, who was much interested, and who thought it was of sufficient importance to justify us in bringing it before the Society. At his suggestion, too, the readings of the R.A. micrometer are added, on which Mr. Robinson's estimates are based.

1921 *April* 7.